

America is represented by a dozen or more species. Now, though an *Erebia* (*E. Tyndarus*, var.) occurs as far south in Europe as the Sierra Nevada, not a single species of any of these three genera occurs in North Africa, although the Atlas Mountains would seem eminently well suited for such Alpine insects. In this case, then, it seems clear that the same cause—the barrier of the Mediterranean—which in the case of the miocene flora of Europe prevented any further retreat south, has operated to prevent any similar southerly spread amongst the victorious invaders from the north which pressed on the retiring host.

With regard to the general similarity in facies and richness between the East American and East Asiatic tree-flora, certain facts pointing in the same direction will at once occur to the zoologist. Thus the *Menopomas* of the Ohio and Alleghany have their only near relations in the gigantic *Sieboldias* of north-east Asia, one species of these occurring in Japan, the other being one of Père David's discoveries in Moupin. Similarly with the genus *Polyodon* amongst ganoids. Only two species of this genus are at present known, *P. folium*, inhabiting the Mississippi, *P. gladius* the Yang-tse-kiang. The recent discovery of at least two species of *Scaphi rhynchus* in Turkestan makes it probable that ere long species of that Americo-Asian genus will be found in the Chinese rivers as well. The parallelism in the case of the salamanders is particularly interesting, when one remembers the celebrated *Andrias Scheuchzeri* of the Eningen beds, and it tends to favour the view that at that time practical identity in the forms of animals and plants reigned throughout the northern temperate zone.

W. A. FORBES

Cambridge, February 14

P.S.—The reported discovery (*NATURE*, vol. xix. p. 351) of a true alligator in the Yang-tse-kiang, will, if confirmed, add a still more remarkable case to those mentioned above.

Leibnitz and the Royal Society

PROF. TAIT and myself ought not to be at issue on this question. I suppose we both want to get at the facts; and, for my part, I have no more desire to whitewash a foul reputation than he can have to blacken a fair one. Where we differ appears to be, as to how far Leibnitz's reputation can stand the test of facts. The question, however, is not whether Leibnitz acted disingenuously in respect to Gregory's series, or any other subordinate matter, but whether he was indebted to something of Newton's, surreptitiously imparted to him, for his differential calculus. If the grounds upon which that charge was made are swept away, there is an end of it. But if, on the other hand, that is not found feasible, and evidence to character becomes a factor in the final decision, then it is right to examine into those subordinate matters. Till then, I, for one, decline to touch them. At the same time let me say that I never undertook to be bail for Leibnitz's impeccability. All I said or say is, that on the published facts I believe that Leibnitz was led to the calculus by his own honest speculations, and had not the means of stealing from Newton, had he been that way disposed. But there are so many relative papers still unpublished, but publishable, that it is impossible to arrive at a true decision till at least some of them have been submitted to an authorised tribunal.

Prof. Tait recommends me to repeat the fruitless attempt of Dr. Slowman. I decline to follow the example of that ominously surnamed *savant*; for it is contrary to precedent that the pursuer should ask the defender to show his hand; and I am quite sure that "the proper authorities" abroad have too much sense to take the initiative. So I appeal to the Council of the Royal Society of 1879 (not that of 1712, as Prof. Tait gives it), and I do so for these two reasons:—

1. The so-called *Commercium* of 1712, which was merely a statement, arriving at no decision on the principal question, contained several allegations (apparently inconsistent with known facts) which give colour to the charge against Leibnitz; it is then an obvious duty on the part of the Royal Society, who were on the occasion represented by the Committee, to give the proof, or make the reparation.

2. The first-published charge against Leibnitz, which was made by Wallis in 1695, was based on allegations said to have been derived from papers and letters in the possession of the Royal Society; it is but fair, then, that those papers and letters should be published.

I therefore once more respectfully urge upon the Royal Society to reopen the main question, and publish such of the relative

papers, &c., in their possession as directly bear upon the original charge.

C. M. INGLEBY

Athenæum Club, February 8

Ear Affection

THE experience of "P." as given in *NATURE*, vol. xix. p. 315, is physiologically interesting, and by no means usual. Before attempting an explanation it may be as well to assume that only one of "P.'s" ears was affected by the disorder, as by this hypothesis we get the greatest possible divergence from the healthy state. It would have been easy to ascertain which was the faulty organ at the time by requesting a musical friend to listen while "P." vocalised the note of the tuning-fork as conveyed to him by each ear separately. The discordant ear would then have been revealed.

The fault of hearing must have been due either to some mechanical misadjustment of the auditory apparatus, by which a wrong sensation was conveyed to the brain, or else to some deep-seated brain or nerve lesion, which led to a faulty conception of the original sound. Let us consider briefly the first of these cases.

From the exceedingly scanty description of his disorder given by "P." I gather that the discord was mostly conspicuous when the note was high pitched (such as when whistled). Now it sometimes happens from paralysis of the chorda tympani nerve, or even from occlusion of the Eustachian tube, that the tension of the ear-drum is preternaturally increased. Such affections, as aurists well know, frequently intensify to a distressing degree the hearing of high pitched notes, whilst they correspondingly diminish the sound of the lower tones of the chromatic scale. This result is probably obtained by the fact that the tense membrane responds more readily to the rapid vibrations of the higher tones than it does to those of a slower rate. We must also remember that the power of lessening the tension of the membrane is in such cases very seriously impaired, and, as a consequence, the power of adjustment also. I do not suppose that in "P.'s" case there was any actual paralysis of the tympanic muscles, but it is just possible that there may have been a certain degree of misadjustment of the drum of the affected ear due to a feeble and imperfect contraction of one or the other of the muscles referred to. If the disorder was, as I surmised, accompanied with great tenseness of the membrane, the laxator tympani would be the faulty muscle. We might, I believe, under such circumstances, expect the ear-drum to vibrate discordantly in response to a note, for Helmholtz's experiments with stretched strings would suggest that this is feasible within certain limits. As a matter of fact this discordance is rare, and therein rests the interest of "P.'s" case.

I can scarcely believe that in his case any of the deeper structures of the ear were seriously implicated, otherwise he would hardly have made such a rapid and complete recovery as he did.

Brighton, February 10

W. AINSLIE HOLLIS

YOUR correspondent "P." (*NATURE*, vol. xix. p. 315) desires an explanation of the phenomenon of alteration in the pitch of sounds, which he has experienced in his own person whilst suffering from temporary deafness. Your second correspondent on this subject, Dr. Wallich (p. 340), was under my observation at the time of his experiencing the same peculiar and comparatively rare aberration, and I was able myself to verify his statements.

I propose with your permission to give an explanation which appears satisfactory to myself, and hope it may be so to your correspondent "P."

Persons suffering in this way find that sounds heard by the affected ear appear to be sharper or flatter than their true pitch as heard by the other ear, and hence a sound may even appear double.

The internal ear, or labyrinth, must be the part affected, and in all probability it is the cochlea which is at fault. Now most authorities are agreed that the pitch of a sound is appreciated by the cochlea in the following manner. Each tone, or division of a tone, has its corresponding portion on the spiral lamina of the cochlea, which under ordinary circumstances can only be affected by that tone. So that the sound-wave produced by a certain tone passes along the keyboard (as it were) of the spiral lamina until it reaches its own key, which it strikes or so affects as to cause an impression to be sent from that portion of the lamina to the brain. Hence the appreciation of variation in the pitch of sounds.

This theory being accepted, for an explanation of the aberration in question we have only to suppose some slight physical alteration in the contents of the cochlea, which would cause the sound wave to strike or affect the wrong portion of the lamina spiralis, and thus a false impression would be carried to the brain.

URBAN PRITCHARD

Now attention is drawn to the above allow me to give another experience.

On two separate occasions while playing the English concertina, and more particularly when single notes or simple chords were struck, I noticed that each was followed by a loud and distinct note an octave lower which appeared to be that of its fundamental tone. The musical tones of the voice of any person addressing me, also, had their deeper reverberations in a similar manner, these being numerous and of rapid succession; the confusion arising was very like that which is heard in a hall unsuitably constructed for sound.

The nuisance, for such it amounted to, I was troubled with for a couple of days each visitation, the abnormal state of hearing being peculiar to the left ear only.

JOHN HARMER

Wick, near Arundel

Intellect in Brutes

THE following case will perhaps interest those who believe that the reasoning faculty in man and animals differs in degree only, and is essentially the same in kind. Some years ago a plumber told me that he had, on several occasions, been called in to examine into the cause of leakage of water-pipes under the flooring of houses, and had found that the rats had gnawed a hole in the leaden pipe to obtain water, and that great numbers of them had made it a common drinking-place, as evidenced by the quantity of dung lying about. The plumber brought me a piece of leaden pipe, about $\frac{3}{4}$ inch in diameter and $\frac{1}{8}$ inch in thickness, penetrated in two places, taken by himself from a house on Haverstock Hill. There are the marks of the incisors on the lead, as clear as an engraving; and a few hairs and two or three of the rats' vibrissæ have been pinched into the metal in the act of gnawing it. This crucial proof of brute intelligence—a rat will not drink foul water—interested me so much, that I ventured to send an account of it to Dr. Chas. Darwin, asking his opinion on the means by which the rats ascertained the presence of water in the pipe. To this he replied: "I cannot doubt about animals reasoning in a practical fashion. The case of rats is very curious. Do not they hear the water trickling?" It may be conceded that this explanation is the most probable, and if it be the true one we have an example of an animal using his senses to obtain the data for a process of reasoning, leading to conclusions about which he is so certain that he will go to the trouble of cutting through a considerable thickness of lead. Obviously man could do *no more* under the same conditions.

ARTHUR NICOLS

OUR ASTRONOMICAL COLUMN

THE COMPANION OF ALGOL.—There are grounds for suspecting that the light of the small star about 80" distant from Algol in the S.P. quadrant is also variable. Schröter in his letter to Bode, wherein he first drew attention to this object, mentions that he detected it with a 7-feet reflector on October 12, 1787, and although small it was distinctly seen. Soon afterwards he estimated its distance from Algol at 1' 30". On April 9, 1788, the star was not to be found, and he therefore concluded that it must be variable. In 1792, when he was in possession of a 13-feet reflector, which he describes as the most powerful instrument then available in Germany, he re-examined the vicinity of Algol, and on March 9 saw the companion much brighter than before, and compares its distinctness in the larger telescope with its faintness in the smaller one with which he had discovered it. But on April 5, in a state of atmosphere at least as favourable as on March 9, with the same instrument and magnifying power, not the slightest trace of the companion could be perceived; on increasing the power to 370, with the utmost straining of the eye, the faintest glimmering was now and then suspected in its position. Schröter then, in this second com-

munication to Bode, expresses himself more confidently as to the variability of the small star.

In the early part of the year 1874 the writer of these lines made several ineffectual attempts to observe the companion, using various powers on a 7-inch refractor; though the skies were favourable enough, nothing could be glimpsed in its place. It was not therefore without surprise that upon re-examining the vicinity under similar conditions on September 9 of the same year, the companion was caught at once, and seen with great distinctness. It was measured with Mr. J. G. Barclay's 10-inch refractor at Leyton, by Mr. Talmage, on October 2 following, when the angle was found to be 194".4 and the distance 79".02; the magnitude was estimated 11.12. An observation by Smyth in 1835 is recorded, but his distance is much too small; it is not stated whether he found the companion himself or whether his knowledge of its existence was due to Schröter's communications to Bode. It does not occur amongst the objects in the "Bedford Cycle," which were re-measured by Secchi.

While upon the subject of variable stars we may just mention that *Andromedæ*, to which attention is directed in the last number of the *Monthly Notices* of the Royal Astronomical Society as "a new variable star," is no novelty: we referred to the star as almost certainly entitled to insertion in the catalogues of such objects, four years since (NATURE, vol. xi. p. 308).

"A MISSING STAR."—From a letter addressed by Prof. C. H. F. Peters, Director of the Observatory, Clinton, New York, to the Superintendent of the Naval Observatory, Washington, which Admiral Rodgers has communicated to the *Astronomische Nachrichten* (No. 2240), it appears that he has strangely misinterpreted a note with the above heading, which was lately printed in this column. We referred to an object observed at Washington, with *Hygeia* in 1850, and afterwards sought for at that observatory and elsewhere on the assumption that it might possibly have been a trans-Neptunian planet, and in view of the failure of a careful search on this hypothesis, we remarked: "the only likely explanation appears to be that there was a variable star in this position, and that the observations in right ascension were affected with greater error than might be expected, considering that on two of the days of observation several comparisons were made." Prof. Peters, however, explains the difficulty by referring several transits to the first instead of to the second wire of the movable plate of the micrometer employed, in which case the star is identified with Lalande 36613, and Prof. Hall has found, on examining the original observing-books, that Mr. Ferguson had altered several correct observations to correspond with erroneous ones, and Admiral Rodgers accepts the explanation as satisfactory. But Prof. Peters is alarmed about the matter now that NATURE "stirs it up again," and writes to the Superintendent of the Washington Observatory "in order that nobody thereby might be induced to spend months and years upon a renewed search," and to "stop any further perpetuation of the credence, that a trans-Neptunian planet is revealed by the Washington Observations." It will be seen that our suggestion was that a variable star might exist in the observed position, and was in no way connected with a renewed search for a trans-Neptunian planet. Prof. Peters must entertain rather odd notions as to the probable knowledge of his astronomical *confrères* respecting the contents of the ecliptical region of the sky, if he believes that any one would be induced, by remarks that we might offer, to undertake in these days a search for a distant planet close to the ecliptic amongst stars of the *ninth* magnitude!

COMET 1871 V.—Dr. B. A. Gould, with his usual energy, has secured an excellent series of post-perihelion places of the comet discovered by Dr. Tempel on November 3, 1871, which in a fortnight's time sank below